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Discussions

Interactive comment on "The wave spectrum in archipelagos" *by* Jan-Victor Björkqvist et al.

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We would like to thank the reviewers and the editorial staff for the swift and timely handling of our submission.

Anonymous Referee 2:

R2: General Comments The authors have analysed a large number of wave observations from a complex shoreline structure, the Finnish archipelago. The paper has many interesting results worth while to publish, like the relation between H1/3 to Hs, the discussion on critical frequency versus peak frequency, how the shape of the spectrum flattens when going more inside the islands. Several interesting parameters are used in the analyse, i.e. the critical frequency from Young (1995), the spectral narrowness parameter κ^2 from Battjes and van Vledders (1984), the degrees of freedom (d.o.f.) from Donelan and Pierson (1983), and the paper has some good results using these,

certainly worth while to publish.

***Our response**:Thank you for putting in the effort to grasp our paper correctly even though it was lacking in clarity. It is greatly appreciated.*

R2: General Comments (continued) But the paper is difficult to read. It needs a considerable rebuilding. Motivation is only vaguely mentioned in between a number of references to different papers showing the authors have done a good research in this analysis. The database seems to have important weaknesses. The data in the inner and sheltered zones have average Hs of around 20 cm decreasing to 2-5 cm. And it seems they are measured with buoys with 40 cm diameter. With sampling frequency 1.28 Hz. And the measuring period is only 14-31 days for these. Can such measurements be at all reliable? The authors do deal in what is a second part of the paper with mostly with T2 versus GoF, where measuring period is 2-3 years. But this should be more clearly stated in the paper.

***Our response**: We have rewritten certain parts of the manuscript for clarity. General motivation has been added to the introduction (page 2, lines 24-30). The approach using only the T2 data has been clarified on page 13, lines 25-30. The discussion part of the paper has been mostly rewritten to focus less on technical issues and instead do more to give the reader a possibility to grasp the meaning of our results.*

The possible problems of the most sheltered locations (S1-S4) were also raised by Referee 1 (please also see our response to that comment). It is true that these measurements are not completely reliable, which can be seen by the mismatch to the other locations in the quantifying numbers in Table 2. This is one of the reasons we used the data from T2, as you pointed out. The short measurement periods are undoubtedly a limitation, but no better data set for archipelagos exist.

We still want to point out that there are two things that increase our trust in that the data is good enough to draw the conclusions we are drawing: i) the transition in Figure 2 presents a gradual and stable transformation of the mean spectral shape. An

instability in these averaged shapes would be a symptom of an insufficient amount of data. ii) the locations T1 and T2 are (geographically) close to each other, and their mean spectral shape are very similar even though T1 is the smallest data set and T2 is the largest. Also the quantifying numbers in Table 2 between T1 and T2 are quite consistent compared to more/less sheltered locations.

We have edited the manuscript to clearly present and discuss the limitations of our data set and how our analysis methods are chosen to minimize the problems (page 12, line 13; page 13, lines 20-30 and page 27 lines 10-18).

R2: Specific Comments The paper should be rewritten for an easier access of results to community. Only a few comments are given here.

***Our response**: We have rewritten the manuscript to the best of our ability by taking into account the relevant comments you have presented. Especially the discussion part of the paper should now aid in giving a better understanding of our results.*

R2: Regarding motivation: It is believed that questions to answer is how much of offshore wave energy enters through the islands, and in what form (distribution in frequency, spectral shape...). Reduction factors are mentioned, without saying how many cases are involved. How is low frequency energy reduced inside the archipelago?

***Our response**: The question is not exactly how the low frequency energy is reduced, but how the propagated low frequency energy compares to the local fetch limited wave system. Even though the energy at the spectral peak in the GoF is heavily attenuated (even to practically 0 for the sheltered location), the relative amount of "long waves" to the local wave system is still larger in the sheltered areas. In other words, in the open sea areas there is no significant contribution from waves longer than the peak. In the archipelago there exists a significant amount of energy in waves that are longer than the local fetch-limited waves. An absolute attenuation goes hand in hand with a relative enhancement.*

While attenuation factors for different fixed frequencies could be calculated, they would not be very informative for the main question of the spectral shape, since the local wave field is not constant throughout the archipelago. Our attempt to capture the relevant features of the spectral transformation is the narrowness parameter. A full description of the problem would require a parameterization of the archipelago wave spectrum, perhaps using the characteristic frequency, the total energy, and some spectral width parameter. This is, however, not a trivial question, which is why it was left unsolved in this paper. We hope to be able to completely resolve the issue of fitting an analytical parameterized function to our data in upcoming studies. This issue is discussed in Section 5.2 of the discussion.

The attenuation coefficients given in Section 2.5 were not meant to be used for any deeper analysis, but were only used to check that our a priori guess of how the sites should be divided up was not completely unreasonable. The results of Fig. 2 and Table 2 further show that the grouping was consistent, and provides a much more elaborate quantification of the spectral evolution than simple attenuation coefficients.

R2: The paper is difficult to read for several reasons. Site description: an overview map is needed for the understanding of fetches. I would suggest one with only land contours, and perhaps the 40m and 80m isolines would help, covering the area of importance for the GOF and T2 point. The map in figure 1 is difficult to 'read' because land has a colour difficult to identify in between strong variations in depth.

***Our response**:Thank you for your suggestion. We think the map in Figure 1 gives a good overview, so we are reluctant to remove it. We have, however, added a blow up of the T2 region with only a couple of isolines (Fig. 2). The land has also been changed to green so that it is not confused with the depth variations.*

R2: The overview of the database in Table 1 comes too late. Names of stations are given in the text here and there, the identifications ('T1', 'T2') would help to be given together with the names.

***Our response**:We have added the reference to Table 1 already in the first sentence of section 2.1 (page 3). We have also moved the place of the table to be earlier in the manuscript, but the exact layout will be decided in production. We have added the identification codes to the names where they were missing in the text.*

R2: Database: What conditions do we have in general at the two sites with wind measure- ments, a) for the 2-3 year period in last part of paper, and in the periods where the inner sites are included.

***Our response**: This information has been provided in the manuscript (page 5 lines 8-9) and as two extra columns in Table 1.*

Other changes in the manuscript:

1) *Page 7: We added a subscript "BFI" to the variables α and β in Eq. (13), since the variable β is used in Eq. (14) in a difference context. The variable α is used in another context in Eq. (19).*

2) *Page 8: We have changed the normalization of the spectra slightly by defining the coefficient β as a mean over $E(f)f^4$ instead of a mean over only $E(f)$. This has no real consequences for the results (which can be seen from the redrawn Figures 2 and 3), but this approach is slightly more flexible. From a theoretical point of view the averaging can now be done over different frequency intervals for each spectrum, if we know that a f^{-4} tail exists. While we use a fixed frequency in this paper, we wanted to introduce this slightly more general method since it might turn out to be useful in later studies, and our upcoming results will then be more consistent with the current ones.*

3) *We added the information that two of the measurement sites were made for research purposes outside the commissioned work by the City of Helsinki. This has no consequences for the paper, but we wanted to represent the data more accurately (page 3 lines 12-14).*

4) *We corrected that the Harmaja wind station is 2 km from the Suomenlinna (T2) wave*

buoy (the Harmaja wave buoy was roughly 5 km from the T2 wave buoy, which is where the incorrect distance stemmed from). (page 5, line 3). This has no consequences for the results of the paper.

5) *Some minor changes to the language and adding sentences explaining the reasoning. These are visible in the track changes version of the manuscript.*

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